Desk
PERIODICAL ROOM
GENERAL LIBRARY
UNIV. OF MICH
JAN 7 1942

THE JOURNAL OF PHILOSOPHY

VOL. XXXIX, NO. 1: JANUARY 1, 1942

CONTENTS

The Status of Prediction: CHARLES E. WHITMORE 5

The Spectator Fallacy: THOMAS A. GOUDGE 14

Book Review 22

Time and Its Mysteries. Series II. Four Lectures given on the James Arthur Foundation, New York University, by Daniel Webster Hering, William Francis Gray Swann, John Dewey, and Arthur H. Compton: ERNEST NAGEL.

Book Notes 24

SEYMOUR G. MARTIN, GORDON H. CLARK, FRANCIS P. CLARKE, *and* CHESTER T. RUDDICK, *A History of Philosophy.*

S. W. FLOSS, *An Outline of the Philosophy of Antoine-Augustin Cournot.*

HELGE LUNDHOLM, *The Aesthetic Sentiment.*

RUFUS M. JONES, *Spirit in Man.*

Proceedings of the American Catholic Philosophical Association, Volume XVI, The Problem of Liberty.

Other New Books and Journals 28

PUBLISHED FORTNIGHTLY

BY THE

JOURNAL OF PHILOSOPHY, INC.

THE JOURNAL OF PHILOSOPHY

FOUNDED BY FREDERICK J. E. WOODBRIDGE AND WENDELL T. BUSH

EDITORS: Herbert W. Schneider, John H. Randall, Jr., Ernest Nagel.

BOOK EDITORS (1939–1942): George Boas, Justus Buchler, Harry T. Costello, John D. Goheen, Albert Hofstadter, Otto F. Kraushaar, Paul O. Kristeller, Kenneth P. Landon, Harold A. Larrabee, Donald S. Mackay, Marcus B. Mallett, V. J. McGill, Ernest A. Moody, P. Romanell, George C. Seward, Jeffery Smith, Norman L. Torrey.

THE JOURNAL OF PHILOSOPHY is owned by the Journal of Philosophy, Inc., and published fortnightly at Prince and Lemon Sts., Lancaster, Pa. Editorial Office: 515 West 116th St., New York, N. Y. President, Herbert W. Schneider; Vice President, John H. Randall, Jr.; Treasurer, Frederick A. Dewey; Secretary, Iris Wilder Dean.

Communications to the editors should be addressed, Columbia University, New York. Subscriptions and advertisements should be sent to THE JOURNAL OF PHILOSOPHY, Prince and Lemon Streets, Lancaster, Pa., or 515 West 116th Street, New York. The annual subscription price (twenty-six numbers) is $4.00. The price of single numbers is 20 cents, except the annual bibliography number, the price of which is $1.00. Advertising rates will be forwarded on application.

Entered as second class matter, March 18, 1921, at the Post Office at Lancaster, Pa., under Act of Congress of March 3, 1879.

PREFACES TO INQUIRY

"A Study in the Origins and Relevance of Modern Theories of Knowledge"

by William Richard Gondin

A freshly inspired work that

...interprets the philosophical works addressed to problems of contemporary research.

...sifts the important from the trivial in the traditional and the new.

...leads to new and challenging problems to be mastered for further inquiries.

Price, $2.00

KING'S CROWN PRESS, Inc.
1145 Amsterdam Avenue, New York City

EDGAR S. BRIGHTMAN, Boston University, says of—

HEGEL'S HELLENIC IDEAL

by J. Glenn Gray

"... I find it not only accurate but really brilliant. Few expositors of Hegel have entered so sympathetically and intelligently into Hegel's essential spirit ... and still fewer have been able to emancipate that spirit from jargon ... I shall recommend [it]."

Price, $1.50

KING'S CROWN PRESS, Inc.
1145 Amsterdam Avenue, New York City

General
Wahr
cop. 1

VOLUME XXXIX, No. 1 JANUARY 1, 1942

THE JOURNAL OF PHILOSOPHY

THE STATUS OF PREDICTION

EVER since prediction and control were proclaimed as the guiding lights of scientific endeavor, it has been customary to assign to the former a prominent place in the progress of knowledge. This is due partly to the general human interest in what is going to happen next, partly to the striking success of certain well-known instances. Indeed, this natural tendency to look toward the future has made "prediction" a familiar word, while the equally logical "postdiction" is not in the dictionaries. Yet it is not usual to ask just how prediction actually works, and just what place it holds in scientific inquiry. Some consideration of these questions with reference to the actual nature and progress of knowledge may therefore be not without interest.

Any body of knowledge which serves as a basis for inquiry gives us a generalized picture of the past and some ground for expectation of the future. In neither direction is it complete to the last detail; there seems no reason to doubt that it does make contact with the actual, but it is never completely and surely adequate. As Professor C. I. Lewis neatly puts it,[1] "*What* I know, is a real object in all its complexity; what I *know,* is merely a limited and superficial set of facts pertaining to this object." Exception might be taken to the term "superficial," which is (and must be) modified as the process of knowledge goes on; but in its main contrast the dictum can and must stand. We do not know the past in complete detail, because much of the evidence is lacking; we can not predict the future in complete detail, because we do not know all the relevant factors; but we can make progress in both directions, so far as our information permits.[2] This would seem to be a minimum statement of the requirements for knowledge in any proper sense.

On this view there is no logical ground for distinguishing between prediction and postdiction; the only reason for beginning with the former is the prominence assigned to it by common con-

[1] This JOURNAL, Vol. XX (1923), p. 146.

[2] For the fundamental position of knowledge of the past, see Professor Lovejoy, *The Revolt Against Dualism,* pp. 305–308; also this JOURNAL, Vol. XXXVI (1939), pp. 486 ff.

sent. As a good starting-point for its consideration, we may take this paragraph from the recent book[3] of Professor Weiss: "There is no systematic knowledge without inquiry and no inquiry without prediction. A prediction is an expectation whose satisfaction is warranted by what is now known. Freed from its ground, it degenerates into a guess and ceases to have an intellectual value. Predictions surprise us only when they are wrong; guesses only when they are right, for to be irrelevant to the discovered is part of the meaning of a guess. Guesses do not surprise us when they are wrong because as we frame them we expect a future having a rational connection with the present, and thus one whose nature is grounded in content other than that expressed in the guess. On the other hand, guesses surprise us when they are right because they then seem to have been pertinent to what we discover and thus fortuitously to contain a ground for future eventualities."

These oracular sentences suggest a number of reservations. First of all, it is probably sound to say that there is no inquiry without anticipation; but to say that there is no inquiry without explicit prediction is to disregard the actual history of science. Many inquirers have been no less surprised than Priestley, when he thrust the glowing splinter into his jar of oxygen "with no expectation of the real issue of it," at what they actually discovered. A long list of such surprises could easily be compiled—the discovery of radium emission by the accidental darkening of a photographic plate, the inversion of temperature in the stratosphere, the superconductivity of metals at very low temperatures may serve as examples. "In science," a recent worker has said,[4] "it is impossible to work directly toward a given large goal, for no one can predict what discoveries may occur along the way, to change and modify the original object."

Moreover, the psychological basis of this account seems more than dubious. The "us" of the paragraph is ambiguous; it presumably refers to the makers of predictions and guesses, but those to whom they are offered should also be taken into account. A sound prediction is often greeted with distrust, an unsound one welcomed. Keeping, however, to the question of the makers, is it the case that predictions surprise us only when they are wrong? It is surely possible to venture a prediction even when the overt weight of the evidence seems to be against it, and yet to be surprised at a favorable outcome. Certainly, from the receiver's point of view, the history of human affairs is full of predictions which caused surprise when they proved to be right. Again, a successful guesser,

[3] *Reality* (1938), p. 91.

[4] Solomon, *Why Smash Atoms?* (1940), p. 58.

who admits that he is only guessing, may be guided by all sorts of intangible considerations, drawn from his past experience, which he could not put into words, but which have some effect in warranting his expectations. It appears that in practice the line between the prediction and the guess is much less sharp than the words of our paragraph would lead us to suppose.

Let us begin a closer analysis by considering two possible sorts of discovery. One is the discovery of a pattern which may be expected to persist or to recur in more or less the same form; the other is the discovery, more or less in advance, of conditions which will produce a certain result, irrespective of any probable recurrence. Corresponding to these are two types of prediction, if they happen to be made: one, that something will be discovered to be the case, and keep on as when discovered; the other, that something will occur at or within a specified time. A good example of the first kind is the working out of the periodic law in chemistry; of the second, the various phenomena of weather—hurricanes, periods of unusual heat or cold, and the like. It is true, as we shall see later, that efforts may be made to reduce the second type to the first by the discovery of some sort of cyclical recurrence; but the doubtful success of such attempts to date justifies us in the provisional conclusion that the two types are really distinct.

In 1815 Prout advanced the conjecture that hydrogen was the ultimate element out of which the others were built. This implied that the various atomic weights were integral multiples of that of hydrogen; but when more accurate determinations showed that this was not always the case, some chemists tried to evade the difficulty by subdividing the weights, while others dismissed the idea as purely illusory. Nevertheless, an uneasy feeling persisted that Prout had got hold of something which could not be dismissed as either illusory or accidental, though no one was able to clarify its nature. Meanwhile, the gradual perception of numerical relations among other properties of the elements led to Newland's law of octaves (disregarded at the time) and finally to Mendeléef's periodic table, which before long became an accepted part of chemistry.

As is well known, the occurrence of certain gaps in the table naturally led to the conclusion that if elements were discovered which would fill those gaps, they would exhibit properties which could be closely predicted from the properties of their neighbors. This was duly verified by the discovery of gallium, scandium, and germanium; and if the multitude of the closely related rare earth metals was somewhat disconcerting, they could be lumped together, and not regarded as infirming a principle of classification which was obviously well founded. For our present purpose, the point

to notice is that the prediction of new elements was based on the existence of gaps within a framework previously worked out. What was not predicted was the existence of elements lying outside the table as then formulated, and yet existing in the very air we breathe. "Until 1894, chemists everywhere believed that dry air, freed from carbon dioxide and incidental impurities, consisted solely of oxygen and nitrogen."[5]

The correction of this belief provided one of the most dramatic episodes in the recent history of science. In 1882 Lord Rayleigh began a redetermination of the densities of the principal gases, partly with the idea of throwing further light on the question of Prout's hypothesis, which still refused to withdraw from the minds of chemists. When, in 1893, it was the turn of nitrogen to be examined, it was found that when obtained from the air it was invariably heavier than when obtained from other sources. Since the discrepancy could not be removed, the next step was to isolate the heavier atmospheric component; and when this was accomplished, in 1894, it proved to be the new gas "argon." It might be supposed that such a discovery would have been hailed with enthusiasm, but that was not the case. Doubts were freely expressed in private, some of them in distinctly unpleasant terms; and even when the gas had been obtained in considerable quantity, another flare-up of skepticism occurred at a meeting of the Chemical Society, and the objections did not subside for some time thereafter, though they obviously lacked justification.[6]

Once the existence of argon had been admitted, the problem of placing it in the periodic table became urgent but difficult, as no obvious vacancy presented itself. Early in 1895 Julius Thomsen printed an addendum to a newly formulated table in which he postulated the existence of a series of inert elements, with atomic weights of 4, 20, 36, 84, 132, 212, and 292.[7] He remarked that he had had the idea *"schon vor Jahren,"* but had refrained from publishing it, "in order not to confuse matters with hypotheses which could not be controlled by experience." In any case, the discovery of helium, announced in March of the same year, made the existence of a new range of inert elements virtually certain, and in 1898 krypton, neon, and xenon were duly found, with weights closely approximating Thomsen's prediction. I do not know how much at-

[5] A. Findlay, *A Hundred Years of Chemistry*, p. 222.

[6] See the *Life* of Rayleigh by his son (1924), pp. 158 ff., and especially Ch. XI, "The Discovery of Argon." The whole affair throws a curious light on the "open-mindedness" of scientists.

[7] *Z. anorg. Chimie*, 9, 283 ff. Radon or niton (weight 222) was discovered in 1900; the last in the series has not yet been attested.

tention this paper received at the time,[8] but it seems to have had no bearing on the actual discoveries made by Ramsay and Travers, which developed naturally out of the previous work.

What, in all this activity, was the part of overt prediction? Not, it would seem, a very large one. Rayleigh seems to have begun his researches without any prediction whatever—certainly with no expectation of finding an unknown gas; nor did Thomsen express his views in print till after argon had been found. After that point, Rayleigh remarked, when Ramsay was about to investigate the chemical properties of the new gas, "I don't believe that it has any," but this was a natural conclusion from the tests that argon had already been put through. The real extent of the range of inert gases had to be settled by experiment, and was attended by considerable controversy before it was so settled.[9] Meanwhile, the discovery of radium, equally unexpected, brought in a fresh set of problems, and the periodic table still fell short of complete clarification. It could accommodate many of the new facts, but it had not predicted their occurrence, and it still presented various anomalies.

The removal of these anomalies was brought about by two fresh discoveries—the first when Soddy, working on the radio-active elements, found that elements having the same chemical properties may have different atomic weights, and named them isotopes, as occupying the same place in the table, and Aston extended the principle to many of the ordinary elements. It at once became apparent that the atomic weights to which so much significance had been attached were really, in most cases, the statistical means of varying proportions of isotopes, and not the fundamental quantities which they had been supposed to be. The other step was Moseley's discovery of the atomic number, denoting the increase of the charge on the nucleus of the atom by regular steps from hydrogen, 1, to uranium, 92. Thus it appears that both the atomic numbers and the atomic mass numbers are integers, just as Prout had conjectured almost a century before. The causes of the slight divergences which occur in actual combinations can be found in the special treatises, and need not detain us here.[10]

[8] I owe the reference to Ruark and Urey, *Atoms, Molecules, and Quanta* (1930), p. 270. It is listed in the very complete account of F. P. Venable, *The Development of the Periodic Law* (1896), but the usual current accounts do not mention it.

[9] "The prediction of the gases of higher atomic weight was more of the nature of an extrapolation than the prediction of the gas intermediate between helium and argon."—M. W. Travers, *The Discovery of the Rare Gases* (1928), p. 81.

[10] A convenient summary is Ch. XIV of F. K. Richtmyer's *Introduction to Modern Physics* (2nd edn., 1934), pp. 614–666.

In the light of all this, shall we call Prout's hypothesis a prediction or a guess? It is evident that the numerical relations on which he based it were incorrect, and the opponents of it were right in rejecting his atomic weights. On the other hand, the computers of these weights were dealing with means which they did not know to be such, the real nature of which it took new discoveries to establish. Yet in the light of these discoveries Prout was more nearly right than his opponents, though his prediction (if we call it that) aroused considerable surprise when it proved to be essentially correct. It seems to occupy an intermediate place between a sheer guess (which it seemingly was not) and a prediction founded on practically indisputable data; perhaps the best course is to call it, as did one of his followers, a "sagacious conjecture."

In this connection, a rather odd view is expressed by Professor Weiss in a passage not long after the one earlier quoted:[11] "Unsatisfactory instruments . . . are devices which occasionally allow one to provide answers which are less radical, complex, and abstract than those required by more adequately presented problems. . . . Had the nineteenth century at its disposal the instruments we have today, it would be almost impossible to make out a table of the elements, for the number of so-called isotopes would have done devastating damage to any simple scheme." In fact, however, the isotopes have nothing to do with the chemical properties of the elements, for they are undetectable by any purely chemical means. Much sounder is the verdict of J. C. Gregory:[12] "Imperative data may make interpretation more difficult by extending its task. They may also make it easier by presenting a recognisable scheme to which it must conform. The Periodic Classification probably eased the construction of the modern 'atom' by directing its planning more than it made that construction difficult by increasing the data."

We may conclude, then, that prediction, like any other feature of knowledge, is subject to casual occurrence and to correction. For example, *what* chemists in 1894 knew was the composition of the atmosphere; but what they *knew* was only its major constituents, and that knowledge, sound as far as it went, did not become a basis for predicting the discoveries that were waiting to be made. Anyone who in that year had predicted that within four years five new gases would be discovered would probably have been greeted with scorn. The whole series of discoveries depended partly on a sound view of the arrangement of the elements as a whole, and partly on the working out of new methods of investigation which themselves were not predictable. *What* the workers on atomic

[11] *Reality*, pp. 94–95.

[12] *A Short History of Atomism* (1931), p. 162.

weights knew was precisely the atomic weights; what they did not *know* was that they were dealing with the means of isotopic atomic masses. In short, the verification of a predicted effect may have to await the making of an instrument capable of detecting it (as was the case, for instance, when Crookes predicted the existence of isotopes as early as 1887), just as the creation of new instruments is likely to result in a flock of more or less unexpected discoveries; and there is always the possibility that an important discovery may be the result of what we can only call sheer accident.

Before leaving this part of our subject, it may be instructive to consider a prediction which was refuted even before it was made. In 1906 Simon Newcomb reprinted an essay [18] in which he set forth the practical impossibility of a heavier-than-air flying machine. "It is quite clear to me," he declared, "that success must await progress of a different kind from that which the inventors of flying-machines are aiming at. We want a great discovery, not a great invention," the discovery to be the work of a great scientist, which the inventor would then modestly adopt; and he concluded, after reviewing the various mechanical objections to the idea, that "the demonstration that no possible combination of known substances, known forms of machinery, and known forms of force can be united in a practicable machine by which men shall fly long distances through the air, seems to the writer as complete as it is possible for the demonstration of any physical fact to be." Yet precisely those forms of machinery and force had, nearly three years before, been used by the Wright brothers to do what Newcomb was demonstrating could not be done.

When we turn to our second class of predictions, those concerning the weather, the state of affairs is very different. The factors of weather—solar radiation, movements of high and low pressure—are fairly well known; the question is, what they will do over a limited area within a specified time. We want to know just when a storm will arrive, or how long a spell of unusual heat or cold is likely to last. Our knowledge of these conditions, in any exact form, does not extend over any very long period; and the specific prediction of changes must often depend on the speed with which observations can be compiled and studied. Those who lived in the area affected by the New England hurricane of 1938 will recall that neither its extent nor its severity was adequately predicted, partly because of the speed with which it moved, partly because of the unusual course it took. The high pressure over Nova Scotia which caused it to swerve from the usual path of such disturbances might

[18] "The Outlook for the Flying-Machine," *Side-Lights on Astronomy*, pp. 330–345.

or might not have served as a basis for prediction; but the time was short, the storm outran the observations, and no such prediction was in fact made.

We all know that there is a general pattern of a year's weather, but that the incidence of its details is varied; hence the desire to find a cyclical recurrence of pattern, for periods which vary from eleven years up. This endeavor, if successful, would of course reduce the second type of prediction to the first; but it has proved exceedingly difficult to find any cycles which can be consistently verified in application to any particular locality. Any extended series of observations is likely to show recurrences which are purely numerical, and have no physical counterpart; and it seems likely that many of the supposed cycles are of this sort. Even if some of them are regarded as real, it has been shown by Brunt[14] that they can account for only so small a part of the observed variations that the question of their reality becomes unimportant. It still remains the case that long-range predictions must either be so general as to have little practical value, or, if more specific, must be frequently contradicted by the actual weather of any particular region.

After all, the essence of prediction is the extrapolation of some feature of present knowledge into the more or less remote future. If we are satisfied with a somewhat sketchy agreement, the principle may be accepted; if we wish to fix an exact date, satisfactory coincidence may or may not occur. It is safe to say that any July 16 will be warmer than the preceding January 16; but to say that any July 16 will be the hottest day of the month is considerably more hazardous. This is true in all sorts of ordinary human affairs, where we may be well assured that a certain thing will happen, but wholly in the dark as to when it will happen.[15] Again, the hurricane of 1938 is a fact; but no clear basis exists for predicting the recurrence of a similar visitation. There is some evidence that a similar storm occurred in 1815, but before that, information is inconclusive or lacking. It would obviously be sheer guesswork to say either that another hurricane would occur about 2060, or that none should be expected to occur before that date. If continuous weather observations ever extend over a period of five hundred years, we shall doubtless know more than we do now; but that is a good while to wait.

[14] D. Brunt, "Climatic Cycles," *Geographical Journal*, Vol. LXXIX (1937), pp. 214–238.

[15] "It is possible to foresee and predict political events with considerable certainty, but very difficult to foresee when they will arrive."—Henry Reeve, in a note to Greville's Diary, under date of February 18, 1838.

As our initial view of knowledge suggests, extrapolation may equally well be directed to the past, the only practical difference being that the issue of a prediction may conceivably be witnessed, whereas the issue of a postdiction, while our experience remains what it is, can not be. Yet astronomers postdict eclipses with as much assurance as they predict them, and historians, biologists, geologists inquire into origins by some projection of our present knowledge into a past which is held in some way to account for what we now know. The geophysicist, for instance, considers such problems as the balance of sodium and chlorine in seawater, the closeness of the moon to the earth in remote times, or the disintegration of radioactive minerals,[16] and thus builds up a history of the earth by interpolation on reasonable assumptions. Unfortunately, there is not complete agreement on what constitutes a reasonable assumption in these matters; speculations as to the past no more command universal assent than do predictions as to the future, and for very similar reasons.

Can we distinguish, in this projection into the past, the two kinds of discovery which guided our discussion of prediction? In a sense, we can; for a good deal of historical research is concerned with fixing the dates of events to a considerable degree of accuracy. As we go back, however, the range of possible application becomes vaguer, until to the biologist, and still more to the geologist, a difference of a million years becomes a small matter, and the emphasis is rather on a general structural distribution than on an exact dating of its components. This state of affairs also suggests that the predictions about which we become most concerned are those which may be fulfilled within our own lifetimes, and that the real cleavage is between such predictions, where accurate dating counts most, and those which will not be settled till a time when we are not there to see them. To examine the relative weights of "short-term" and "long-term" expectations would be another task. It is unlikely that the span of human life measures the logical cogency of all the constituents of our knowledge; but there are certainly practical differences in our experience, whether or not they are abolished in the long run.

Thus the cases we have examined lend no great support to the view that there is no inquiry without prediction, or that a prediction is necessarily warranted by what we now know. Prout's conjecture was not warranted by what was known in his time; but it served as a notable ferment, since some chemists were spurred to

[16] See H. Jeffreys, *Earthquakes and Mountains* (1935), pp. 96 ff., for a concise account of these and similar questions; also, from a somewhat different angle, A. Holmes, *The Age of the Earth* (1937).

more accurate determinations of atomic weights by their efforts to disprove it, and others, reluctant to dismiss it altogether, made it the basis for further speculations. Yet it took a full century to establish its essential truth by means which were undreamt of when the discussion started; and Prout himself might well have been astonished at the form it ultimately assumed. It seems fair to conclude that the rediscovery of a previously discarded notion, the attempt to justify a formal prediction, and the accidental revelation of the unexpected may all contribute to the advance of knowledge, and that among them explicit prediction does not occupy a privileged position.

CHARLES E. WHITMORE.

HINGHAM CENTER, MASSACHUSETTS.

THE SPECTATOR FALLACY

THERE is an ancient and widely accepted opinion that a philosopher is properly "the spectator of all time and all existence." This has been interpreted to mean that his primary relation to the world is one of detached contemplation. He is by profession an onlooker or spectator; and it is only in terms of such an attitude that he can theorize successfully about the nature of things. Now this opinion I hold to be fallacious. Not only is it in conflict with undeniable facts, but its acceptance has given rise to a number of problems that appear quite artificial when approached from a more adequate standpoint. In what follows I propose to state why I regard it as both erroneous and harmful. For the sake of convenience I shall refer to it as "the spectator fallacy."[1]

I

A basic consideration may be introduced at the outset by way of the distinction between theory and practice. Taking these terms in a functional sense, "theory" will be understood to refer broadly to the activity of thinking or reflection; and "practice" to muscular or motor activity, particularly the sort that effects some change in features of the immediate environment. Under the latter heading are included commonplace processes like eating, drinking, walking, manipulating objects, etc., as well as the elaborate experimental procedures found in natural science. Now the point I wish to stress is that *practice is always primary and*

[1] This designation will immediately suggest Professor Dewey's criticism of "spectator" theories of knowledge (cf. *Essays in Experimental Logic; The Quest for Certainty;* etc.). My remarks, however, are directed against a rather different doctrine.

theory secondary. This is true in several important senses. But the one to be discussed here relates essentially to the life of the individual.

Every human being has during infancy lived through a pre-reflective period when the requirements of practice were paramount. And these requirements continue to be fundamental, though they are greatly modified and extended, after the capacity for thought has developed. Thus, even the most leisured philosopher is obliged to meet the demands of practical activity from the cradle to the grave. As a biological organism he must eat, dress, and attend to his other bodily needs. In doing so he has to move about and utilize objects of various kinds. As a social being he must enter into a number of active relations with his fellows; and so on. In short, whether he indulges in philosophical reflection or not, he can never refrain from acting. Like everyone else, he is first and foremost an agent. For the majority of mankind, indeed, whose very existence depends on the performance of physical work, the point needs no arguing.

To recognize the primacy of practice implies no disparagement of theory. It is quite compatible with the view that activity should whenever necessary be controlled and directed by thought. As a matter of fact, the practice of human adults always does involve *some* theory, though the latter may not be explicitly formulated or even recognized. Where this happens the danger is that the two processes will become divorced from one another, so that a theory will be framed which is entirely at variance with what is actually being done. Hence the desirable situation is one in which the processes constantly interact. Only when practice gives rise to a clearly recognized theory that in turn influences practice can their relation be fruitful. Furthermore, save in the case of purely formal disciplines like logic and mathematics, theory must ultimately be validated in terms of practice. The one reliable test of existential truth is operational. These traits are best exemplified in the methodology of modern science, where steady progress is achieved by the interplay of theory and practice all along the line. At the same time, practice (in the form of experiment and observation) remains the basic part of the whole enterprise.

It is when theory and practice get out of gear that baneful consequences ensue. The spectator fallacy is one of these. Because philosophers have tended to divorce their professional speculations from their practical conduct, they have been able to write and speak as though they were perfectly detached contemplators of the world, rather than primarily agents in it. The re-

sult is that they have often propounded theories completely inconsistent with the basis on which their own (and everybody else's) ordinary behavior takes place. The history of philosophy provides many examples of such theories, which Professor Broad has rightly referred to as "silly." A silly theory, as he points out, is "one which may be held at the time when one is talking or writing professionally, but which only an inmate of a lunatic asylum would think of carrying into daily life." Extreme behaviorism and certain types of idealism are mentioned as cases in point. "No one in his senses can in practice regard himself or his friends or enemies simply as ingenious machines produced by other machines, or can regard his arm-chair or his poker as being literally societies of spirits or thoughts in the mind of God."[2] To call such doctrines "silly" is not to cast reflections on the intelligence of their authors. It simply signifies that the latter have unwittingly fallen victims to the spectator fallacy.

A few philosophers have been candid and self-critical enough to take note of this situation in their official pronouncements. Hume does so in a familiar passage in his *Treatise*.

> I am at first affrighted and confounded with that forlorn solitude in which I am placed by my philosophy. . . . For I have already shown, that the understanding, when it acts alone, and according to its most general principles, entirely subverts itself, and leaves not the lowest degree of evidence in any proposition, either in philosophy or in common life. . . . Most fortunately it happens, that since reason is incapable of dispelling these clouds, Nature herself suffices to that purpose, and cures me of this philosophical melancholy and delirium, either by relaxing this bent of mind or by some avocation . . . which obliterates all these chimeras. I dine, I play a game of backgammon, I converse, and am merry with my friends; and when, after three or four hours' amusement, I would return to these speculations, they appear so cold, and strained, and ridiculous, that I cannot find in my heart to enter into them any further. . . . Here, then, I find myself absolutely and necessarily determined to live, and talk, and act like other people in the common affairs of life.[3]

The obvious conclusion for Hume to draw here is that something is radically wrong with his philosophy. He does virtually admit this when he characterizes it as "cold, and strained, and ridiculous." Yet he makes no attempt to revise his philosophical doctrines so as to account for the fact that in the ordinary affairs of life he is obliged to "live, and talk, and act like other people."[4]

[2] *The Mind and its Place in Nature*, pp. 5–6.

[3] *A Treatise of Human Nature*, Vol. I, Bk. I, Pt. iv, Sec. vii.

[4] It is true that he refers to "custom," "habit," "natural instinct," etc., as the factors operating at the commonsense level. But he never integrates these with his official philosophy which is formulated wholly in terms of "impressions" and "ideas." Cf. *Treatise*, Bk. I, Pt. III, Sec. viii; also *Inquiry*, Sec. v, Pt. 1.

The main reason for his failure to undertake the revision, I suggest, is that as soon as he begins to theorize he unconsciously adopts the attitude of a spectator to whom practice *as such* is alien. Practical activity is then either completely ignored as of no philosophical importance, or else is treated in terms of "impressions" which present themselves serially to the mind. Both these alternatives are profoundly unsatisfactory. The first just increases the separation of theory and practice by omitting to consider a vital area of human life. The second distorts the nature of practical activity *as it is for a person engaged in acting.* No one *qua* agent merely experiences series of sense-impressions, kinesthetic sensations, etc. Practice is something unique and *sui generis,* which is seriously misrepresented by being thus "reduced."[5] Leaving aside for the moment the question of giving an adequate conceptual account of practice, I think it is fair to say that the account offered by Hume (and the majority of philosophers) is defective because it deals with action not as it is for an agent, but as it is for a passive spectator.

Since practice is the primary mode of activity, the plane on which it predominantly functions must be equally primary. That plane is the everyday world in which we all live, move, and have our being. The theory (or set of beliefs) generated by activity in this realm is usually spoken of as "the common-sense view of the world." Its major tenets have been given a precise logical statement by Professor G. E. Moore in his penetrating essay, "A Defence of Common Sense."[6] Roughly speaking, such beliefs as the following are involved. There is a physical world existing quite independently of anyone's perception or thought. This world consists of a vast multitude of material objects, events, and persons, located in space and time, and related in various ways. The objects on or near the earth's surface have shape and size in three dimensions, as well as qualities of other kinds like color, temperature, etc. All the things in the world are subject to change which takes place with varying degrees of rapidity, so that some exist for a very short time while others are relatively permanent. Every human being has a physical body born at a certain time in the past, and governed by certain laws which determine its growth, mature functioning, and death. During his lifetime, each person acts, per-

[5] Peirce is one of the few philosophers to stress this point. In his phraseology, it is completely incorrect to reduce Secondness (action-reaction) to Firstness (quality) or to Thirdness (thought). Secondness is an irreducible element in the world, with its own specific nature. Cf. *Collected Papers,* 1.355ff., 1.426ff., 2.84ff.

[6] *Contemporary British Philosophy,* Vol. 2, pp. 193–223.

ceives, thinks, hopes, feels, dreams, remembers, and so on. The physical world as a whole has a long history which antedates the appearance of any human being; and it manifests a number of uniformities which make successful predictions possible. In spite of this, however, an element of the unexpected or of novelty is always to be found in things.

Now I wish to argue, in general agreement with Professor Moore, that this common-sense view of the world is in fact accepted, either implicitly or explicitly, *by everybody*. Even those philosophers who treat it with contempt in their professional utterances do really assent to it when they stop theorizing and begin to act. Like the "plain man" they have no choice in the matter. Hence, philosophers don't differ from one another at the level of practical action, for all of necessity adopt the common-sense view. Furthermore, I think Professor Moore is correct when he declares that this view is in certain fundamental features *wholly true*. But where he says that we simply "know" its truth directly and immediately, I contend that we know it to be true *because* it emerges from and is constantly being verified by practice. Professor Moore does not consider the validating function of practical activity in this connection. But to me it seems all-important. I do not suggest for a moment that the common-sense view represents the whole truth about the world. Many other aspects, some of them extremely intricate and obscure, have been and will be discovered by the refined types of practice employed in the natural sciences. But no discoveries of this sort can be validly used as evidence that the world of ordinary experience is "unreal" or "mere appearance." One of the major tasks of philosophy is to produce a synthesis of scientific and common-sense theory, recognizing that each is true in its own context of verification.

II

As a supplement to the foregoing, I want to consider two specific issues which seem to me to have arisen in philosophy because of the spectator fallacy. They are (*a*) the doctrine of solipsism, and (*b*) the epistemological problem of perception and the external world. Due to their origin, both issues appear largely artificial in character, as I shall endeavor to show by touching on them very briefly in turn.

Solipsism. Practically all philosophers in the past have agreed that solipsism is an indefensible doctrine. If a theory could be shown to lead to solipsism, that was taken to be decisive proof of its falsity. Recently, however, for reasons into which we shall not enter, solipsism has been reinstated as a tenable theory, and

is even in certain quarters explicitly defended. Thus, Mr. Russell has declared that "if inference is taken in the sense of strict deductive logic, there is, so far as I can see, no escape from the solipsist position."[7] In a similar vein Professor Stace writes that "all knowledge must have had its beginning in my own self-enclosed personal experience. This original solipsism is utterly inescapable except by prejudice or refusing to see it."[8] And as our final sample, we quote Professor Dingle: "To say that solipsism is logically irrefutable is to express the fact far too mildly. It is not merely irrefutable; it is obviously true."[9]

If by an effort of imagination I adopt the attitude of a completely immobile spectator, it is possible, perhaps, for me to find solipsism momentarily credible. But the instant I discard this pose and begin to *act* in any way, the absurdity of the doctrine becomes patent. My "make-believe" vanishes into thin air as soon as I pick up my pen or rise to answer the telephone. Likewise, if in my philosophizing I fail to take account of the basic rôle of practice, it is possible for me to elaborate a "theoretical" defence of solipsism. To this extent Mr. Russell is right when he asserts that from the standpoint of "strict deductive logic," i.e., pure theory, the doctrine is inescapable. But that is not to say that it is factually true, for the factual truth of a theory can only be guaranteed by practice. Thought alone is able to certify formal consistency, nothing more. Hence when we apply the test of practice to solipsism, we discover (what everybody knows) that it is obviously false and preposterous. It simply does not arise except for those philosophers who commit the spectator fallacy.

Perception and the External World. A great deal of subtle and painstaking analysis has been expended on the epistemological problem of perception and the external world. It may therefore sound presumptuous to suggest that the problem itself is in large measure gratuitous. But that is the conclusion I feel inclined to defend for reasons sketched below.

The customary analysis of the problem runs somewhat as follows. We start with a perceptual situation registered by a proposition like "I am seeing a penny." The question is then raised whether the direct sensible appearance is literally part of the surface of the penny, in the way that common sense takes it to be. A negative answer is suggested on the basis of evidence drawn from physiology and physics. This is supported (or preceded) by arguments making use of the fact of perceptual illusion. The up-

[7] *The Analysis of Matter*, p. 398.
[8] *The Theory of Knowledge and Existence*, p. 67.
[9] *Through Science to Philosophy*, p. 345.

shot is that we are forced to distinguish between the sensible appearance (sense-datum, etc.) and the external material thing. But the latter is something that can never enter into a perceptual situation. What logical grounds have we, then, for believing that such a thing exists at all?

This question has never been answered satisfactorily. Even the most competent epistemologists have been driven to offer the lamest of answers to it. Thus, Professor Broad, after pointing out that both deductive and inductive reasoning fail to yield the necessary proof, declares that the concept of a material thing is just a "postulate"—an "innate principle of interpretation which we apply to the data of sense-perception."[10] Professor Price goes even further when he remarks near the close of his comprehensive survey of the problem that "a pure physical object is something so shadowy that we can scarcely conceive of it at all."[11] Because of the inconclusiveness of such replies, Professor Stace and others have urged that the notion of a physical object *per se* is an unjustifiable assumption which ought to be dropped. Objects can then be defined phenomenalistically in terms of sense-data alone.[12] This does seem to me to be the logical terminus of the argument. But it seems equally clear that no one is a phenomenalist in practice. No one in the course of every-day activity treats his pipe, or his golf-clubs, or his own body, or any other physical object, as a set of "permanent possibilities of sensation" or a "family of sense-data." The ludicrous effect created by using these expressions to describe ordinary conduct is further evidence of their inadequacy. I conclude, therefore, that the above analysis must be defective at some crucial point.

That point, I contend, is the very beginning. The kind of perceptual situation initially selected for scrutiny is the relatively uncommon one in which we sit and gaze passively at an object. But as we have been maintaining, man's primary relation to the world is that of an agent, not an onlooker. We do not customarily *contemplate* pennies, pipes, golf-clubs, etc.; we *use* them. It is through action that we come into contact with the external world, and perception is essentially a means of facilitating this. More-

[10] *Op. cit.*, p. 217.

[11] *Perception*, p. 303.

[12] Cf. *The Nature of the World*, pp. 117ff. Also, A. J. Ayer, *The Foundations of Empirical Knowledge*, Ch. V. Mr. Ayer presents his version of phenomenalism as "linguistic" rather than "ontological." It is due, he says, solely to a preference for the sense-datum language rather than the material-object language. Yet he declares that the necessary and sufficient condition of the existence of sense-data is that they should actually be sensed (pp. 71–72). This is surely an "ontological" remark!

over, since practice determines the truth of theory, the common-sense belief in the existence of physical things is as true as a belief can possibly be. It can not be discredited by an analysis which starts from the highly artificial standpoint of a pure spectator. I should suppose that every epistemologist would admit that in his non-professional moments he has an "instinctive belief" in perceptual realism. But he usually refuses to recognize that this belief is far more solidly grounded than most of his academic opinions.

What has been said in no way controverts the fact that perception involves certain difficult problems of its own. But these belong to the science of psychology. Being factual, they can be handled effectively only by experimental research. Significantly enough, epistemologists have generally failed to make use of the attested results of psychological inquiry. As Professor Wood has recently remarked, they tend "either to appropriate unconsciously outmoded psychologies of the past, or else to improvise psychological theories to meet the exigencies of their epistemological speculations."[13] When the problem of perception is allocated to the proper discipline, it is seen to be concerned exclusively with *how* we perceive the external world of physical things, events, and persons. That there *is* such a world is amply guaranteed by our practical activity. Here again the philosopher is faced with the task of achieving a synthesis of psychological, physical, and common-sense theory, accepting the contextual truth of each so far as it has been verified by practice.

The spectator fallacy, I have been arguing, is one exemplification of the divorce of theory and practice. Consequently, if philosophers strive to keep these processes integrated, the fallacy will not arise. Theory will be theory *of* practice; and practice will be practice *of* theory—as in the case of modern science. Neither process, however, can legitimately be reduced to the other. For the individual this means that the phenomenon designated by the expression "practical activity" is not something which thought alone can grasp. Actual participation is also required. Granted the latter, an adequate conceptual account of practice would be such as would enable the individual to identify the various phases of activity as he engages in it, and so to bring it under conscious direction and control. But further discussion of this complex matter must be reserved for some future occasion.

THOMAS A. GOUDGE.

UNIVERSITY OF TORONTO.

[13] *The Analysis of Knowledge*, p. 5.

BOOK REVIEW

Time and Its Mysteries. Series II. Four Lectures given on the James Arthur Foundation, New York University, by DANIEL WEBSTER HERING, WILLIAM FRANCIS GRAY SWANN, JOHN DEWEY, and ARTHUR H. COMPTON. New York: New York University Press. London: Humphrey Milford. Oxford University Press. 1940. viii + 137 pp. $2.00.

The present volume contains the annual lectures delivered on the James Arthur Foundation during the years 1936 to 1939. The addresses by the three physicists ("The Time Concept and Time Sense among Cultured and Uncultured Peoples" by the late Professor Hering, "What is Time?" by Dr. Swann, and "Time and the Growth of Physics" by Professor Compton) deal with the development of chronometry and with significant innovations in both technology and physical theory arising from refinements in the measurement of time. Although these essays are informative, they are not free from dubious history (as when Professor Compton credits Galileo with the experiment of dropping balls from the tower of Pisa) or from the perversity of offering mystification and paradox as enlightenment (as when Dr. Swann discusses relativity).

Professor Dewey's address on "Time and Individuality" deals with more general themes. His thesis is that development is the mark of individuality and that "temporal seriality is the very essence of the human individual." Moreover, in opposition to the view that sequential change is foreign to the nature of inanimate things, he maintains that "the principle of a developing career applies to all things in nature." Two corollaries are drawn from this metaphysical conclusion. The first is that progress is not inevitable, but is contingent on the efforts of men to control change in a given direction; consequently, the ground of democratic ideas and practices must be a faith in the potentialities of individuals. The second is that art complements science in being a manifestation of individuality as creative of the future; hence, since "the free individuality which is the source of art is also the final source of creative development in time," the regimentation of artists is treason to the better future which such regimentation would serve.

Professor Dewey's essay is provocative, but his argument is puzzling. He rests his case concerning the universal validity of the "principle of a developing career" on two main contentions. The first is that "scientific objects" are purely relational, so that the method of physical science is concerned with the measure-

ment of change and "not with individuals as such"; the second is that it is becoming clear that in physics "statements of what actually occurs are statistical in character as distinct from so-called dynamical laws that are abstract in character, and disguised definitions." Professor Dewey seems to me correct in his characterization of scientific objects. But is it really the case that the methods of the natural sciences are not concerned with "individuals as such"? Do we not have reliable physical knowledge concerning such manifest individuals as the sun, the earth, and even individual human beings? The second contention appears to me even more dubious. I will not stop to argue the question whether the theories and laws of mechanics, classical thermodynamics, relativity theory, and a good part of optics and electrodynamics are in fact statistical laws, and if not whether they are disguised definitions. I do not think that the answer is affirmative in either case. The crucial question is whether, even if the statements of physics are statistical in character, it follows that development characterizes all things in nature. It does not seem to me that this consequence can be validly drawn; for there is no contradiction in using both a statistical and a non-statistical ("dynamic") theory for the same subject-matter, as is evident from the fact that both statistical mechanics and thermodynamics can be employed in the domain of thermal phenomena.

Professor Dewey also appeals to the Heisenberg indeterminacy relation as evidence for the fact that "the individual is a temporal career whose future can not be *logically* deduced from its past," so that "for physical individuals time is not simply a measure of predetermined changes in mutual positions, but is something that enters into their being." Waiving the question whether the proposed interpretation of the Heisenberg principle is adequate, it is amusing to find Professor Dewey denying that science is concerned with the intrinsic natures of things while at the same time he makes statements which do just that. Moreover, while the predictions of science are only probable, as Professor Dewey rightly maintains, this fact is not a specific outcome of the Heisenberg principle; for it is justifiable to assert that fact even for the classical dynamical theories, so long as those theories have an empirical content and are not "disguised definitions." It is not clear, therefore, what relevance the Heisenberg principle has for Professor Dewey's argument.

Professor Dewey maintains that a thing's potentialities are not fixed and intrinsic to it, but depend on the consequences of its interactions with other things. It is somewhat strange, however, to find him also maintaining that the validity of this doctrine is

essentially related to a view which regards the "spatial rearrangement of what existed previously" as in some sense an inferior manifestation of development than a "qualitatively new" happening. For since we can not tell what the character of physical theories will be like after another hundred years of research, this seems like giving needless hostages to the future. But what I find really strange, in the light of Professor Dewey's main thesis, is to have him declare that an individual can lose his individuality by becoming imprisoned in routine, that "our behavior becomes predictable because it is but an external rearrangement of what went before," and that the human problem is the control of change in a given direction. Since this problem, if it is to be solved, presumably involves making predictions concerning the future on the strength of past behaviors, these various doctrines do not seem to me compatible. I do not think, therefore, that the metaphysical underpinning Professor Dewey supplies for his democratic faith is an adequate or, for that matter, a needful structure. And I think that in attempting to supply it in the manner of the present essay he has not been faithful to his own best teachings.

ERNEST NAGEL.

COLUMBIA UNIVERSITY.

BOOK NOTES

A History of Philosophy. SEYMOUR G. MARTIN, GORDON H. CLARK, FRANCIS P. CLARKE, CHESTER T. RUDDICK. New York: F. S. Crofts and Co. 1941. xi + 546 pp. $3.25.

At his death Professor Martin left the plan and some preliminary studies for a textbook in the history of philosophy. His literary executor, Professor E. A. Singer, Jr., as he explains in the foreword to this book, delegated the task of completing the projected work to three men, specialists in their respective fields of ancient, medieval, and modern philosophy. Except for the stipulation, in accord with Professor Martin's desire, that "the history should be short rather than long, and thorough in a few things rather than summary in many," and the inclusion of Professor Martin's own chapter on the Pre-Socratics, the elaboration of the details of the volume was left in the hands of the three collaborators.

The insistence that the book be brief and concise has given it its particular character and value. The authors have tried to present in most cases the essentials of a given doctrine without following its various ramifications or the application of it to particular fields. Plato, for example, is presented first in terms of his relation to Socrates and the setting of his problem, then in terms of the

theory of ideas and of Platonic science. The task of broadening the scope of the study and elucidation with regard to individual dialogues is left to the instructor.

The book covers the history of philosophy from the Pre-Socratics through Kant. The authors explain in an epilogue that there is no significance in the fact that it stops there; it simply had to stop somewhere. They have been wise in not aspiring to completeness, which in works of this kind usually involves numerous painful and meaningless one-sentence descriptions of the positions of more minor figures. From ancient philosophy Mr. Clark has chosen the Pre-Socratics, Plato, Aristotle, the Stoics as a group, Sextus Empiricus, representing the sceptics, and Plotinus, as presenting Neo-Platonism. Of the medievals Mr. Clarke gives some background in Philo Judaeus, and then discusses Origen, Augustine, and Dionysius for the patristic period, Erigena, Anselm, Abelard, and Bonaventure as Augustinians and Thomas, Scotus, Ockham as Aristotelians. Mr. Ruddick uses Copernicus, Bruno, and Montaigne as transitions to the modern period and then includes Descartes, Spinoza, and Leibniz as rationalists, Locke, Berkeley, and Hume as empiricists, concluding with a chapter on criticism and Kant.

Each author makes extensive use of quotations from the source material and appends a brief bibliography of primary and secondary materials. The intensive rather than extensive treatment of the theme makes the book much more flexible and adaptable than the general run of textbooks on the history of philosophy.

G. C. S.

An Outline of the Philosophy of Antoine-Augustin Cournot. S. W. Floss. Philadelphia: Privately printed. 1941. 168 pp.

A doctoral dissertation presenting Cournot's philosophy by means of copious quotations from his writings bound together by explanatory and sometimes expository paragraphs. In view of the rarity of Cournot's books in American libraries and the paucity of articles upon his works, this volume should prove valuable as an introduction to his interesting philosophy.

G. B.

The Aesthetic Sentiment. A Criticism and an Original Excursion. Helge Lundholm. Cambridge, Mass.: Sci.-Art Publishers. 1941. 223 pp.

This is one psychological study in esthetics that is decidedly not psychologistic! The author recognizes three problem regions

in axiology: (a) the relation of man to value, (b) the relation of value to any particular type of value, and (c) the relation of man to the particular type. This psychological study concentrates upon (c); but Lundholm contends: "sharp separation of problems of these contrasted types is untenable."

Samuel Alexander's relational theory of value is accepted as the philosophical orientation that is most in accordance with research of the proposed type. Another orientation point is Lundholm's own psychological theory—perhaps best expressed in his *Psychology of Belief* wherein the Stout-McDougall tradition is purged of alien elements, and carried through to its essential implications as a pure conationalism.

The Aesthetic Sentiment contains sections on the formal appeal of art and on the affective appeal. The former include comparison of the esthetic and the logical "moments" of contemplation and of these with non-contemplative experience. Unity in the art object is said to involve mutual reinforcement between an affective dominant form and affective subordinates.

Lundholm questions certain features of Alexander's theory of esthetic creation. As Lundholm is himself an artist, and son of a novelist, he writes with experience here, at least when describing immediate experiences.

The theory that esthetic acts involve "sublimation" is accepted, but quantity of released tension is regarded as irrelevant to criteria for normative judgments of the object. And this is not the only juncture at which a difficult balance between the various types of problem is successfully maintained. In the literature of psychological esthetics Lundholm's book is distinguished alike for its broad perspective, and for its wisely restrained, well-considered claims.

ROBERT F. CREEGAN.

CUMBERLAND UNIVERSITY.

Spirit in Man. RUFUS M. JONES. Stanford: Stanford University Press. 1941. xi + 70 pp. $1.25.

The reasonable grounds for a philosopher's faith in immortality are presented in the three chapters of this book. Admittedly immortality can not be demonstrated (p. 64). But does this present life reveal anything that leads naturally to faith in immortality of the spirit in man? By spirit the writer means a conscious self, an experiencing subject. The body is regarded as obviously not immortal (p. 4). The spirit is characterized by "free ideas" which are explicit thoughts independent of what is given by sense experience. These free ideas are the basic unities and forms through which we interpret all our experiences. They enable us to antici-

pate and handle situations not yet experienced. They provide the "more yet" which carries our minds beyond any given situation. And yet they do not act independently of the external world of nature. The unity of experience is fundamental. Subject and object are two aspects of one whole (p. 20).

When a mind of the spirit type operates in mutual and reciprocal relation with a real world of nature, with no dualistic chasm, Dr. Jones believes that there is at once a basis for values of experience. He describes the great realities of the Good, the Beautiful, and the True as breaking through the material medium into the spirit and achieving a mental significance. Thus Beauty involves an object that finds us and presents something that in its patterns or shapes is the way it ought to be (pp. 34, 35). It is as objective as it is subjective. Such a realm of values involves a "more-yet" and he believes that we must take by faith as real the kind of world in which values can exist (p. 46). In the light of our own little over-world he believes it is a safe prediction that there is an over-world of a higher order. Minds of our type can have come from no lower source than Mind of this higher order (p. 53). He rests his case on experience and not logic and concludes that we must wait for confirmation until we burst through the gates of the new life.

K. P. L.

Proceedings of the American Catholic Philosophical Association, Volume XVI: The Problem of Liberty. CHARLES A. HART, Editor. Washington, D. C.: The Catholic University of America. 1941. 293 pp. $1.50.

This volume contains ten papers read at the sixteenth annual meeting of the American Catholic Philosophical Association, held at Detroit in December, 1940, as well as abstracts of six Round Table Discussions and various reports and minutes of the Association. The ten papers are devoted to the problem of human liberty, which is approached in pretty much the same way by each contributor; that is, as a problem of the reciprocal determination of human will and intellect, and of the mode of dependence of the human act of choice on the all-embracing determination of existence by the will of God. Such differences of opinion as may be detected in the papers represent varying shades of emphasis on the relative primacy of will or intellect. The papers were presented by the following: Professors A. C. Pegis, Berard Vogt, Joseph B. McAllister, Gerald B. Phelan, Gerard Smith, Yves Simon, Hunter Guthrie, Francis E. McMahon, Geoffrey O'Connell, and John McCann.

In the Round Table Discussions, signs of an awakening recognition of contemporary logic are revealed in the treatment by Sister Miriam Theresa Rooney of "Law and the New Logic," and in Father Philotheus Boehner's discussion of Ockham's treatise on future contingents in connection with the problem of a multi-valued logic. Ockham, according to Father Boehner, considered that Aristotle's denial of determinate truth or falsity to future contingent propositions implied recognition of a third truth-value; though the discussion is interesting, it is far from convincing to the present reviewer.

While there is much of incidental interest in the various contributions to this volume, the total effect is not exciting. It would seem to be an inevitable consequence of the *de jure* acceptance of the thomistic determination of all the basic issues of philosophy, as a norm, that questions of fundamental philosophic import are excluded from controversy or criticism, so that the problems open to debate tend to be of secondary or trivial character. Yet it should not require great profundity to see that if such had been the condition of philosophic inquiry in the thirteenth century, there could not have been a Thomas Aquinas.

E. A. M.

OTHER NEW BOOKS AND JOURNALS

JOURNAL OF THE HISTORY OF IDEAS. Volume III, Number 1. Causation in Historical Events: *F. J. E. Teggart.* Causation and its Application to History: *M. R. Cohen.* Causal Analysis in History: *Maurice Mandelbaum.* The English Virtuoso in the 17th Century (I): *W. E. Houghton, Jr.* The Leadership Principle in National Socialism: *M. M. Ball.* Thomas Hood's Inaugural Address as First Mathematical Lecturer of London (1588): *F. R. Johnson.* Book Review, Peter Viereck's Metapolitics: From the Romantics to Hitler: *Jacques Barzun.* Reply: *Peter Viereck.*

THE NEW SCHOLASTICISM. Vol. XVI, No. 1. The Principle of Order in Politics: *Wilfrid Parsons.* Some Comments on the Nature of Mathematical Logic: *J. J. Wellmuth.* On the Platonism of St. Augustine's *Quaestio de ideis: Hans Meyerhoff.* The Reality of Matter in Creation: *L. J. Eslick.*

THE MODERN SCHOOLMAN. Vol. XIX, No. 1. Consistent Materialism: editorial. Plato's Concept of the Philosophic Life: *R. V. Schoder.* Prologue to Evolution: *E. T. Foote.* Causality and Evolution: *G. P. Klubertanz.*